

Putting the Pieces Together

Explore the idea and purpose of developing a research agenda for Ed Tech.



Glen Bull's call for a research agenda for one-to-many computing (see p. 42) raises a larger question: what is the status of a research agenda within education technology?

Let's consider first what a "research agenda" is in general. The Merriam-Webster dictionary defines research as "studious inquiry or examination." (There's more to the definition than that, but this is a good starting point.) It further defines *agenda* as "a list or outline of things to be considered or done." So if we want a research agenda in Ed Tech, we need to make a list of, say, options for or types of studious inquiry or examination of education technology.

By Robert Kadel

Subject: Ed Tech research

Standards: NETS•A II (<http://www.iste.org/nets/>)

Supplement: <http://www.iste.org/ll/>

We are certainly not alone in considering this issue. Try going to Google and typing the words "research agenda" into the search box. Hit Enter, and you'll get the usual millions of hits that Google can find these days. Looking at the first page, you'll see that research agendas have been proposed in disability data, occupational research, information architecture, Parkinson's disease, and music education, among others.

So why not us? Certainly one of the most ridiculed but common reasons for doing something in research is because "everybody else is doing it." But in this case, what everybody else is doing seems to be on the right track.

I recently wrote about research on teacher attitudes regarding technol-

ogy integration into instruction (*L&L* February 2005, pp. 34–35, 47), and in this article I mentioned that no one researcher can answer a broad research question. It takes a community of scholars who all contribute their valuable research—pieces to a large puzzle that is the answer to that broad research question.

So a research agenda can help guide that community of scholars. A researcher in any of these other, highly varied fields benefits from having a specific vision or a set of goals for the types of research that will help his or her field advance more efficiently and effectively. Furthermore, a research

agenda helps to outline particular steps to be taken and particular methodologies to be used. (Yes, I say "methodologies"—plural—and I'll expand on this shortly.)

John Cradler's Research Windows column from the April 2003 issue of *L&L* (pp. 54–57) summarizes CARET's (Center for Applied Research in Educational Technology, <http://caret.org>).



iste.org) opinion, based on reviewing more than 1,200 studies, that there is a whole host of problems in how Ed Tech research has been conducted. Beyond that list, though, Cradler and CARET identify nine goals that must be accomplished within Ed Tech research to help educators plan and use technology effectively. For example, the research community must ensure that assessments are directly related to and valid measures of curriculum standards, both those provided by the state and those addressed by the software being used. This seems like a given, but you'd be surprised (or perhaps you wouldn't) how many research projects don't actually address the issues they're supposed to be studying—neither by asking the right questions nor by using a methodology appropriate to what is being studied. (*Editor's note:* You can find Cradler's article in *L&L*'s archives at <http://www.iste.org/l1/>.)

And yes, despite the buzz that emanates from Washington, D.C., a number of methodologies are appropriate to Ed Tech (and all

educational) research besides experimental/quasi-experimental design. Depending on how far a research question has progressed through the community of scholars, case studies, interviews, focus groups, surveys, and so on are needed. All of these can illuminate central questions that can then be addressed by randomized, controlled trials in an experimental design. Anything less than choosing the methodology appropriate to the question at hand is irresponsible. And, in my opinion, there should be adequate state and federal funding for any of these methodologies if it is suitable to the question at hand.

In volume 36, number 1 of the *Journal of Research on Technology in Education* (pp. 60–76), M. D. Roblyer and Gerald A. Knezek move us that much closer to a national Ed Tech research agenda by proposing four topics that should be central to what we study in this field. The first, "research to establish a relative advantage," focuses on establishing which technology-based methods of instruction can provide *unique* and *consistent* benefits. That is, we need to know what types of tech-based instruction provide a one-of-a-kind benefit not found elsewhere, and that these benefits are seen across a range of different student populations. (*Editor's note:* Read Roblyer and Knezek's article in the online supplement.)

The second topic, "research to improve technology implementation methods," seems self-explanatory, but allow me to clarify. A number of types of technology are already being used in schools or have the potential to be used, from word processors and graphing calculators to electronic

whiteboards and highly sophisticated curriculum management systems. Research is needed to determine the most effective methods for implementing these vast and varied resources. (In addition, don't forget, this research needs to be unique and consistent; see above.)

Roblyer's and Knezek's third topic? "Research to monitor technology's impact on important societal goals." Because of my sociology background, this one is my favorite. For example,

we need to know how technology in education is helping to close the gap in achievement scores between students from low-income households and those from high-income households.

Finally, Roblyer and Knezek propose that we conduct "studies that monitor and report on current technology uses to help us shape desired directions." The key term here is *desired directions*—we need research that looks at how we currently use technology to help us understand the ways we should use it in the future. What do we want to accomplish in educational technology? Well, like so many other things in life, it is easiest to determine where we want to go by first looking at where we are.

Bull's article this month meets a number of the criteria discussed above, and he should be commended for it. Now it is time for the rest of us to throw our hats into the ring—we are all a part of this community of scholars and we must work together to bring about the research, and ultimately the changes, that we all wish to see in our schools.



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New Millennium Research For Educational Technology: A Call for a National Research Agenda

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Abstract

Research to indicate unique pedagogical benefits of technology in education flourished until the mid-1980s, when criticism of the premises and methodologies of studies made educators question the usefulness of so-called "media research." Currently, increasing costs of maintaining technology infrastructure and evidence of low use of technology by teachers has generated new urgency for research to help provide a rationale for specific uses of technology to support teaching and learning. The new National Educational Technology Plan currently under development by the U.S. Department of Education provides an ideal opportunity for articulating a plan for meaningful research. This article describes topics and issues on which new research should focus and offers guidelines for research methods to address previous criticisms and provide results to guide future use of technology in education. (Key-words: educational technology, instructional technology, educational technology research, research methods, research agenda, technology impact studies.)

INTRODUCTION

Three assumptions educators often make about research on technology-based (i.e., computer-based and online distance learning) methods are that: (a) it has a commonly-held, theory-based agenda, (b) findings provide convincing evidence about the unique ways modern technologies enhance achievement and motivation, and (c) these findings shape practice in the field. The last thirty years of educational technology research notwithstanding, none of these assumptions are currently true. The authors propose that current conditions in education and society offer the combination of urgent need and unique opportunity to make all of these assumptions realities. A review and analysis of the history of recent research on electronic technologies in education reveals key issues and problems that must be addressed as we contemplate a research agenda for the next era in technology use in education.

BACKGROUND ON THE EDUCATIONAL TECHNOLOGY RESEARCH AGENDA

As Richey (1997) pointed out, agenda-building in a field as prone to change as educational technology is a "complex process ... often dependent on far more than whether or not an idea is worthwhile" (p. 8). Yet the importance of what she calls a "systemic theory agenda" cannot be overstated. It "captures the

intellectual climate of a field ... identifies those issues currently receiving major attention ... is supported by influential scholars of the day ... is heeded by expert practitioners ... influences the selection of articles for publication in journals of our field (and) paper proposals accepted for presentation at our national conferences ... and shapes university curricula which prepare and update the professional in our field" (p. 7).

Until the early 1980s, the theoretical foundations of educational technology research were not widely debated and the research agenda based on them seemed comparatively clear. The theory base was primarily cognitive-behaviorist, and research into technology-based methods reflected this view. In addition to numerous evaluations of computer-based and distance education applications and products, there were many experimental and quasi-experimental "media comparison" studies comparing technology-based with non-technology-based methods to establish which one was better for teaching a topic or skill. A thriving meta-analysis culture had the goal of identifying trends in findings across these studies. The question common to all these enterprises was fundamentally a cognitive-behaviorist one: "How effective is a technology in helping learners acquire desired behaviors (knowledge and skills demonstrated by an observed and measured performance)?"

Attacks on this solid bastion of traditional thinking came from two directions, one theoretical/pedagogical and one methodological. Working together, they constituted an assault that effectively called a halt to media comparison studies and meta-analyses and left the field without a clear philosophical framework or workable research agenda.

The first attack came from educators, psychologists, and researchers who espoused a constructivist perspective on learning. They said that the aim of education was not to transmit learned behaviors from an expert source (an instructor or a package of instruction) to students. Instead, it was to create environments in which students generated their own knowledge with appropriate assistance or "scaffolding" from teachers. Seymour Papert (1980) was an early and outspoken representative of this perspective, which he based primarily on the theories of Piaget. Later, the Cognition and Technology Group at Vanderbilt (1991, 1993), calling on work by Vygotsky in cognitive scaffolding and Brown, Collins, and Duguid (1989) in situated cognition and cognitive apprenticeships, became a major hub of constructivist research and development. These theorists and researchers turned the focus away from the impact of the technology being used to the impact of "anchored instruction," which technology could support.

At about the same time, Clark's (1983, 1985, 1991) criticism of the research methods used in studying technology-based approaches began a parallel attack on the traditional research agenda. Clark said that "media are delivery vehicles for instruction and do not influence learning" (1983, p. 453). He based this on analyses of past media research, which he found to have uncontrolled effects due to methods and content. For example, he found that two methods being compared often had different teachers. He concluded that any effects attributed to a technology were, in fact, caused by the methods used by the teacher or other variations in the content of the treatments. Based on these issues, he called for a moratorium on media research.

Kozma (1991) tried to address both theoretical and methodological problems by agreeing partially with Clark. Kozma said that, as long as media were viewed strictly as delivery systems, there would be no discernible difference among treatments that was not due to confounding factors. Instead, he proposed a theoretical framework that “presents an image of the learner actively collaborating with the medium to construct knowledge” (p. 179). He felt that the “symbol systems” and “processing capabilities” unique to each medium could make an identifiable and useful contribution to this collaboration. While advocating more research to clarify these unique contributions, he left the guidelines for this development and research up to the “creativity of designers” (p. 206) and those who would study their products.

Kozma’s proposal was helpful in redirecting the focus on studying the contributions of the technology-based method, rather than the technology or “medium” itself. Subsequent research such as the well-regarded *Technology Counts* study (Archer, 1998) seemed to be heeding this line of reasoning, asserting that it was not how much teachers used computers but how they used them that made a difference in students’ achievement.

At the same time, pragmatism born of experience with students and classrooms dictates that we modify pure constructivism as a theory basis for a new research agenda. In his call for a “post-modern agenda for instructional technology (IT),” Soloman’s (2000) insights are useful. He maintained that “The philosophical core of post-modern IT is a belief in pluralism, which can be described as respect for difference and resistance to single explanations … pluralism in our field posits that there is no single best model or theory of learning” (p. 15). He suggests that the sheer complexity of the learning context calls for a closer look at the combination of method, learner, and environmental variables that contributes to successful learning.

In Soloman’s “post-modern” view, both “scientific” and “critical” methods must be used to study this complex picture. Science is “problem-driven and goal-based” (p. 16), and looks at whether objectives have been achieved. Criticism looks at the details that could account for the outcomes. Like many current theorists, Soloman clearly emphasizes the constructivist view of learning as “knowledge (being) created rather than found” (p. 16), but his acknowledgement of the need for scientific study allows for the statement of specific problems to investigate and measurable observations of changes in behaviors: two conditions the cognitive-behaviorists have always said should be the basis for establishing that learning has indeed occurred.

The merging of critical features of these heretofore competing theories sets the stage for a research agenda to study the benefits of technology-based methods as integral components of solutions to instructional problems, rather than as “delivery media.” Although we recognize that this proposal will not (and probably should not) silence the ongoing debate about the principles and practices of cognitive-behaviorist versus constructivist approaches to learning, we do feel that some détente in the theory debates is essential if we are to progress to the work that must follow them: crafting and studying technology applications that work in actual instructional settings.

THE NEED AND OPPORTUNITY FOR A “NEW MILLENNIUM” RESEARCH AGENDA

The need for a new research agenda has never been more urgent. Recent projects under the U.S. Department of Education’s (DOE’s) *Preparing Tomorrow’s Teachers to use Technology (PT³)* initiative have demonstrated clearly that the process of integrating technology effectively into education comes with a high price tag. Educators and policymakers look to educational technology research to help supply a solid rationale for why these funds are well spent (Ringstaff & Kelley, 2002). This is increasingly important in light of recent criticisms of the continuing investment required for technology infrastructure and the inconsistent impact and low usage by teachers, regardless of their increased training and access to resources (Cuban, Kirkpatrick, & Peck, 2001).

Cradler (2003) stresses that the recently enacted No Child Left Behind (NCLB) Act (2002), which will be influential in the next few years, requires that learning resources such as electronic technologies “be closely aligned to state academic standards and that research document the extent to which (they) result in student achievement gains as measured by standards-based assessment” (p. 55). However, in his review of “best evidence” studies posted by the Center for Applied Research in Technology (CARET), a funded project of the International Society for Technology in Education (ISTE), Cradler found that electronic learning resources aligned to less than 40% of the California Content Standards for the major content areas. He also found a “shortage of formal controlled research” (p. 56). Even with experimental studies that reported statistical significance, there was “very little information on the educational significance or the implications for teaching or learning” (p. 56).

In addition to the NCLB requirements, the DOE is currently calling for input from educators to create a new National Educational Technology Plan (www.nationaledtechplan.org). As Richey (1997) observed, another component of an agenda-setting process is “support of credible and authoritative people in the field ... The larger the base of support, the more likely a topic is to become part of the discipline’s systemic agenda” (p. 9). The DOE’s National Technology Plan could be the single most influential force on the educational technology research agenda for the future. It is crucial that the important topics to be studied are articulated now and offered as an integral part of the plan.

FOCUS AND TOPICS FOR A FUTURE TECHNOLOGY RESEARCH AGENDA

Recommended Focus: Providing a Rationale for Technology Use

In light of the pressure from policymakers and from within the field itself, the primary task of the new research agenda seems clear. Future research must address squarely the question of why teachers should use technology-based methods. The emerging theory base demands that studies look at technologies not as delivery systems, but as components of solutions to educational problems, and that research questions be stated in a way that the contributions of methods can be examined and tested.

Roblyer and Schrum (2003) say that rationale-building research should concentrate on the “relative advantage” that technology-based methods and materials offer over other methods and materials. This concept is based on Rogers’ (1995) diffusion of innovations research that shows potential adopters of a technology are more likely to accept it if they see clear benefits or “relative advantage” of new methods over old ones.

In his talks to groups of educators around the country, former PT³ Program Director Tom Carroll (2000) emphasized that some technologies in common use have obvious relative advantage that needs no research to confirm it. For example, when computer simulations save schools the costs of consumable materials or allow multiple simulated experiments that are not feasible using actual environments, relative advantage should be readily apparent. However, even methods with self-evident benefits are in need of explication to realize their potential in practice. In addition, other methods have been hypothesized to have unique instructional benefits and some have popular support, but research to confirm these benefits has been inconsistent or done with inadequate controls or numbers of subjects to make findings useful to others.

Recommended Topics: Important Issues to Address

This dual view of technologies as both an already-accepted part of some areas of educational practice and as a potential influence in others requires a multifaceted approach to providing research that can guide both policy and practice. We propose that the studies that would be most helpful are those intended to indicate: (a) that certain technology-based methods have the potential for unique and fairly consistent benefits in response to certain kinds of educational problems; (b) there are ways to implement technology-based methods already in widespread use (e.g., word processing, distance learning) that lead to increases in their impact on achievement, retention, and student satisfaction; (c) progress we are making to meet some of education’s most important goals for technology (e.g., decreasing the Digital Divide); and (d) how we can shape and direct our technology resources to better accomplish their intended purpose. These purposes and examples of studies that address them are described briefly here.

Research to establish relative advantage. In their review of findings to date on the impact of technology-based methods on achievement, Ringstaff and Kelley (2002) find that research has provided considerable evidence that directed methods such as using software tutorials for self-paced instruction and drill and practice for enhancing retention of learned skills can significantly raise scores on standardized tests. However, even this evidence is not as helpful as it could be, because studies tend to focus on “a specific software or technology, severely limiting the generalizability of their results ... and fail to take into account the larger classroom, school, and district context in which the technology was used” (p. 6). Ringstaff and Kelley also acknowledge that evidence of benefits frequently proposed for more constructivist, inquiry-based uses is also “far from conclusive” (p. 7). Research is needed to indicate that the following often-proposed benefits are possible:

- The graphics and interaction features of modern visual technologies have unique qualities to increase and speed comprehension of unfamiliar or abstract topics by clarifying their underlying concepts.
- Visual examples provided by problem-solving software, simulations, and video scenarios prevent inert knowledge by linking the concepts to real-world applications. (Brown, Collins, and Duguid, 1989, describe inert knowledge as skills that students learn but never use because they cannot see where or how they apply to problems they encounter in real-life situations.)
- Online collaboration projects encourage greater appreciation of diverse cultures and improve reading and writing skills.

Studies that address these kinds of claims are beginning to appear. For example, Doty, Popplewell, and Byers (2002) looked at whether the interactive qualities of CD-ROM storybooks could result in greater comprehension for students than reading in a static format, and Funkhouser (2002–2003) explored the impact on achievement of constructivist instruction made possible by the unique capabilities of geometry software, as opposed to the traditional instruction that non-electronic materials are capable of accomplishing.

Research to improve technology implementation methods. A casual reading of any professional journal or education periodical reveals that many technology-based methods are already in widespread use (e.g., word processing, distance learning). Research is urgently needed to help shape the ways in which these technologies are being implemented to increase their impact on established measures of education quality: achievement, retention, and student satisfaction. Even accepted “older” technology-based solutions such as using word processing for composition writing have not been sufficiently studied in practice. In reviewing evidence on how people grade word-processed vs. handwritten products, Roblyer (1997) found that reviewers who had not been trained to guard against a bias tended to give word-processed products significantly lower scores. An example of recent research into commonly-used online methods is Overbaugh’s (2002) study of how electronic mailing lists and bulletin boards could be implemented more effectively to take advantage of unique features while preventing unintended outcomes. Findings from such studies have important implications for future practice.

Research to monitor technology’s impact on important societal goals. Although some technology uses in education require more research to justify them, access to technology is acknowledged as being an essential part of a quality education for citizens in today’s Information Society. We also need to know that increased access to technology is resulting as expected in greater learning opportunities and skills for all students. Studies are needed to monitor our progress in accomplishing the following kinds of important goals:

- Technologies are helping underserved students gain increased access to learning opportunities.
- Ready access to unique information sources and populations is enhancing critically important information literacy skills.

- More use of visual technologies is increasing critically important visual literacy skills.

Useful studies in this area are often surveys such as those by Hoffman and Novak (1998) to capture the impact of race on Internet access and computer usage. Others are studies such as the one by Chisholm, Carey, and Hernandez (2002), who reviewed access to and uses of information technologies by students of various ethnic backgrounds.

Studies that monitor and report on current technology uses to help us shape desired directions. Finally, research is needed to shape the characteristics of our newer technology resources and guide their uses in education. Mioduser, Nachmias, Lahav, and Oren (2000) examined the characteristics and usefulness of educational Web sites to determine how they should be better developed and implemented in the future. Harris and Jones (1999) studied communication patterns of online mentoring to determine how this kind of activity could be better implemented to increase impact. Research is needed to help shape appropriate uses of these and many other technologies (e.g., handheld computers) beginning to appear with increasing frequency in schools.

CRITICISMS OF PAST RESEARCH AND GUIDELINES FOR FUTURE METHODS

Clark's (1983, 1985, 1991) criticisms of the focus and methods of technology research are not the only negative evaluations the field has received. The usefulness of findings in this area has long been compromised by a reliance on funding from the very groups who, as Clark pointed out, would profit most from positive findings: hardware and software companies. There is also consistent evidence of what can only be characterized as basic design and methodological flaws in many studies. Much of educational research in general is often the target of the latter criticism (Kaestle, 1993). However, technology research is perhaps in a rather more tenuous position than many other areas of education, in that it has long been in need of a rationale sufficient to justify its high costs and complexity. Guidelines for future research are needed that recognize these flaws and offer means of remedying them.

Guidelines for Research Hypotheses

Clark (1983) cited design flaws common to individual studies (e.g., different teachers for experimental and control groups) and the meta-analyses that summarize findings across them (e.g., tendency for publications to accept primarily studies that report significant, rather than non-significant, differences). Kozma (1991) and others acknowledge that a research agenda based on showing the "overall impact of one medium over another across learners" (p. 204) will not guide us into the new millennium of technology in education. However, Clark's criticisms and Kozma's acknowledgement expose a far deeper failing inherent in much of educational research: flaws in the ways hypotheses are (and, often, are not) thought about and posed.

It is far from clear in many studies that researchers have considered carefully why given methods are being proposed for testing. It is axiomatic that educa-

tional research should help address heretofore unresolved or poorly resolved educational problem(s), and that hypotheses should be derived from a theory base that leads logically to the hypothesis as a solution to the problems(s) and makes it clear why it should be tested.

Guidelines for Design Methods

In his review of recent “best evidence” technology research, Cradler (2003) alludes to several kinds of design flaws, some of which indicate underlying problems with hypotheses. He says that it is often “not clear what specific research and evaluation questions the study is attempting to answer” (p. 56) or its “educational significance or … implications for teaching and learning” (p. 56). Other design flaws he cites include: small sample sizes that reduce statistical significance; measures of impact unrelated to objectives of the materials; measures of impact that are not objective and/or validated; descriptive, non-quantitative methods only with no control or comparison groups; and self-report data unaccompanied by corroborating data.

Not only must future research refrain from simplistic comparisons of materials cited by Clark, it must begin to focus on the unique qualities of technologies to make possible given methods. If a study proposes that a technology-based method is superior, it should be hypothesized to be superior because it has unique characteristics to solve a significant, unresolved problem such as low reading comprehension (as addressed by the Doty, Popplewell, and Byers, 2002, study) or students’ persistent difficulties with geometry concepts, as addressed by Funkhouser (2002–2003). Studies must offer evidence—both in design and in data analysis—that the symbol systems and/or processing capabilities of a technology make possible an instructional method or learning environment that would not be possible through another (i.e., non-technology) method.

Because the NCLB Act will have a long-term impact on all future educational research methods, it is important to acknowledge the criticism of past methods implicit in its recommendations. It is clear that so-called “scientifically-based research (SRB) methods” mentioned more than 100 times in the NCLB Act reflects a pervasive impatience with the failure of past research to provide objective, helpful evidence to guide practice. Cradler (2003) recommends that, in addition to using multiple data sources and validated measures, studies should set up control groups whenever possible and use valid comparison groups even if true controls are not possible.

It seems important to join those who, in the aftermath of the NCLB Act, have asserted that the failure of past research cannot be remedied solely by using experimental and/or quasi-experimental methods. Berliner (2002) reminds us that one of the largest and best-funded replication studies ever, one of different instructional models of early childhood education, found that variation was larger across different sites implementing the same program than it was across different programs. “No program could produce consistent results across sites. Each local context was different, requiring differences in programs, personnel, teaching methods, budgets, leadership, and kinds of community support” (p. 19). Thus, he says, “good science” requires not only trying to capture valid and reliable evidence of improvement, but

also attempting to explain the complex set of conditions that led to improvement. As Solomon (2000) recommended, critical analysis methods must join "scientific" ones to help us understand what works, when it works, and why.

Reporting Guidelines

Cradler's (2003) review of past research found many reporting flaws that are just as problematic as design ones, because they limit both interpretation of findings and studies designed to replicate them. Reporting flaws include: insufficient information on the intervention and/or technology and other resources being implemented and the characteristics of large samples that allow disaggregation of important variables; and a disconnect between the conditions under which technologies are used and their conclusions, or between the conclusions and the data cited to support them.

Reports of research studies must address these frequently-cited shortcomings. They must be more comprehensive and informative about the methods and materials used, conditions under which studies take place, data sources and instruments, and subjects being studied; and they must emphasize coherence between their methods, findings, and conclusions.

CONCLUSION: IMMEDIATE AND LONG-TERM PRIORITIES

To explore the degree to which current research may already be addressing the four kinds of needs cited here, we analyzed the purposes of articles in the 1999–2003 issues of the *Journal of Research on Technology in Education* (*JRTE*). Findings from this analysis (Table 1) revealed that most articles focus on evaluations of local programs or on methods of furthering technology integration or improving teacher training. Our analysis provides further evidence of what Cradler has already found. Relatively few studies are being done for purposes that would help justify the costs of integrating technology-based methods.

As Table 1 indicates, most published research articles describe methods of getting students or faculty more involved with a technology (e.g., the Internet) or how to structure training and other conditions to get them more interested in using technology in general. From the perspective of the NCLB Act, this kind of research begs the question of "Why should we have students or faculty use technology more?"

It is to be expected the National Education Technology Plan now under development will provide considerable and immediate impetus to focus on these and/or similar topics and to improve the quality and usefulness of research. Hopefully, this plan also will make it easier to obtain funding from sources other than those that could be perceived as having a self-interested agenda.

However, groups Richey (1997) describes as the base of support for a technology research agenda will have the most direct, long-term impact on how well studies that support a new agenda are carried out: influential scholars and expert practitioners who guide dissertation research, select articles for publication in journals of our field and paper proposals for presentation at our national conferences, and who shape the curricula of teacher preparation programs. These groups will help shape the next millennium of technology in education as

Table 1: An Analysis of Article Types in *JRTE* from 1999–2003

Types of Articles	Number of <i>JRTE</i> Articles 1999–2003
Type #1: Studies or reviews of unique benefits of various technology-based methods compared with non-technology-based methods	11 (8%)
Type #2: Studies on ways to increase impact of technologies already in common use	2 (1%)
Type #3: Studies on trends in usage related to achieving important societal goals	8 (6%)
Type #4: Studies that monitor/report current technology uses to help shape desired directions	5 (4%)
Type #5: Other studies, e.g., evaluations or descriptions of programs, implementation methods, or usage characteristics; types, extent, or perceptions of integration	87 (64%)
Type #6: Theoretical articles and position papers	23 (17%)

they view proposed studies or reports of studies through the lens of whether they address one of these key, unmet needs and meet design and reporting requirements. It is they who will determine that research findings are meaningful and helpful to those who look to them for guidance.

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Answering the Call: A Response to Roblyer and Knezek

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First, I'd like to express my gratitude for the opportunity to respond to the Roblyer and Knezek article. I believe that it raises some critical issues about formulating a much-needed research agenda for educational technology and I appreciate being part of the conversation.

Let me begin with the authors' assessment of current educational technology research—that it tends to lack sufficient theory, fails to provide adequate evidence about how modern technologies enhance achievement and motivation, and fails to adequately shape practice in the field. That's a strong statement, but unfortunately, I tend to agree. I do appreciate, though, the positive manner in which these issues were framed, focusing on the challenges and opportunities that we face. It should be noted that designing and implementing technology-based learning activities in schools can be likened to learning to fly an airplane while it is being built. Furthermore, if you factor in the numerous competing agendas for school reform it is not surprising that the research agenda for educational technology is not particularly focused. Considering the progress that's been made in the field in a very brief amount of time, we should not be overly critical of our shortcomings. With that said, though, I agree with the authors that as our field matures, and as external pressures mount, it makes sense to formulate a more focused research agenda that will better inform practice in our schools.

The historical perspective that Roblyer and Knezek provide is very helpful for informing where we currently are as well as where we should be going. Although I tend to agree with Clark (1983, 1985, 1991, 1994, as cited in Roblyer & Knezek, 2003) in his criticism of media comparison studies as they were conducted, I accept Roblyer and Knezek's premise that the goals of such research should be revisited. Perhaps part of the problem has been the proliferation of many small-scale studies that failed to acknowledge the range of variables involved in technology-based learning, and in effect, resulted in overly simplistic comparison of media. I agree with Kozma's (1994) conclusions, published in a special issue on the topic, that we need to conduct research on "what ways can we use the capabilities of media to influence learning for particular students, tasks, and situations" (p. 18). That fits nicely with Roblyer and Knezek's premise that we should "look at technologies not as delivery systems, but as components of solutions to educational problems" (p. 65).

As the authors suggest, it makes sense to reach "some détente in the theory debates" (p. 64) and to proceed with an agenda that helps inform what works in actual instructional settings. Furthermore, it is reasonable to assert that technology-based methods should ultimately be able to demonstrate a "relative advantage" over other instructional methods. In this case, though, I would like to em-

phasize the word ultimately, and suggest that we investigate relative advantage in a balanced, forward-looking way. With the NCLB legislation and accompanying views of assessing student achievement, relative advantage for some could easily be reduced to quantitative measures. Although it's clear that standardized tests and other objective measures are a force to be reckoned with and should be considered and thoughtfully employed, I would challenge the research community to continue exploring the relative advantage of transformative technology applications that may not fit as readily with the status quo. It seems clear that we need a balance of research that informs "what to do on Monday" with research that may ultimately inform what we do on Monday five or ten years hence. A broad interpretation of relative advantage seems critical to accommodate interest in "established measures of education quality" as well as the transformative vision that Seymour Papert (1980) articulated with Logo two decades ago or what Alan Kay (2003) is exploring with Squeak today.

The concept of relative advantage can also be applied to particular approaches within an instructional method. For example, Bernie Dodge (2003) recently suggested five models of motivation that could inform a research agenda investigating approaches to the design and implementation of WebQuests. Although Dodge was not particularly interested in research that compares WebQuests to traditional approaches such as lectures, he did suggest that theory-based research was needed to help identify approaches within WebQuests that increase student motivation and learning.

I appreciate Roblyer and Knezek's suggestion that studies should seek to identify technology-based methods that "have the potential for unique and fairly consistent benefits in response to certain kinds of educational problems" (p. 65). Although this may be a matter of semantics, I prefer this language to the more prescriptive phrases of "best practice" and "scientifically-based evidence." For me, such phrases tend to minimize the range and importance of variations in particular educational contexts. Teaching is both an art and a science, and as we seek to inform those elements that elevate the science of teaching, we should also seek to dispel the notion that effective teaching and learning can be reduced to science alone. The authors did well to cite Berliner's notion that "good science" requires not only trying to capture valid and reliable evidence, but also attempts to account for the complex set of conditions that led to improvement.

GUIDELINES FOR DESIGN METHODS AND REPORTING

With the caveat that research topics should remain sufficiently balanced to address a range of research questions, I support Roblyer and Knezek's call for greater rigor within all approaches to research. It makes sense that research questions be stated in a way that the contributions of particular methods can be examined and tested, and that researchers seek to address the design shortcoming outlined. I agree that "they must be more comprehensive and informative about the methods and materials used, conditions under which studies take place, data sources and instruments, and subjects being studied; and they must emphasize coherence between their methods, findings, and conclusions" (p.

69). Although we clearly should refrain from simplistic comparisons of materials cited by Clark (1983, 1985, 1991, 1994, as cited in Roblyer & Knezek, 2003), I also agree that research needs to help us sort through the myriad approaches to using technology and help identify those “promising practices” that may consistently be effective in addressing particular educational needs. While qualitative studies can be particularly helpful in exploring new approaches and applications, as Berliner (2002) reminds us, good science requires detailed descriptions of the context and implementation strategies employed. Without including such descriptions in research reports, the transferability of such findings to other contexts is severely limited (Lincoln & Guba, 1985).

One goal of the proposed new millennium research agenda would be to inform decision making in terms of the allocation of funds to support technology-based methods. This, of course, is reasonable, with one important caveat. Part of effectively implementing technology in schools depends upon identifying and establishing the essential conditions to support optimal implementation. In many cases researchers have put forth conclusions about particular technology-based methods without adequately scrutinizing critical factors such as the adequacy of student access to the technology, the comfort level of the teacher, etc. I would argue that researchers must help identify the conditions necessary to realize the relative advantage of a technology-based method and avoid making premature conclusions about particular methods-based studies in which essential conditions are not in place. This supports Roblyer and Knezek’s notion that educational technology research needs to help discern fairly consistent benefits of particular technology-based methods. Without paying more attention to the necessary conditions, we will be much less likely to ascertain and document a method’s relative advantage.

For example, Hall and Hord (2001) provide a model for studying change efforts that takes into account the concerns and levels of use of the implementers, as well as how the innovation is configured when implemented. They state that a major reason widespread change is often not successful is that change implementers do not fully understand what the innovation is and what it will look like when put into practice. Furthermore, they explain, “When a variety of configurations of an innovation are implemented, there is little likelihood that significant gains in student learning will be detected across all classrooms” (p. 53). The researchers recommend creating an innovation configuration map that identifies major components of innovations and then describes observable variations for each component. Such measures fit well with the need to define what effective technology integration looks like so that we might be able to discern positive learning outcomes and the relative advantage that may result.

Providing this level of detail will enable researchers to obtain more valid and reliable data to inform policy debates and classroom practice. A look at the well-publicized debate between Larry Cuban and Hank Becker may prove useful to this discussion. As Roblyer and Knezek noted in their article, Cuban and colleagues have

articulated forceful arguments questioning the benefits derived from learning technologies in schools (2001, as cited in Roblyer & Knezek, 2003). In response to Cuban's long standing arguments, based on his recent research, Becker wrote:

Thus, in a certain sense Cuban is correct—computers have not transformed the teaching practices of the majority of teachers, particularly teachers of secondary academic subjects. However, under the right conditions—where teachers are perfectly comfortable and at least moderately skilled in using computers themselves, where the school's daily class schedule permits allocating time for students to use computers as part of class assignments, where enough equipment is available and convenient to permit computer activities to flow seamlessly alongside other learning tasks, and where teachers' personal philosophies support a student-centered constructivist pedagogy that incorporates collaborative projects defined partly by student interest—computers are clearly becoming a valuable and well-functioning instructional tool (2000, p. 29).

I think Becker's statement nicely illustrates the nature and complexity of educational change with technology and points to the challenges that await researchers in the field. For those of us who have experienced the magic of teaching and learning with technology and "know" of its benefits, it is incumbent upon us to document those approaches to inform policy and classroom practice. Toward that end, I applaud Roblyer and Knezek's efforts to lay out some critical issues and put forth an agenda that will help us accomplish these goals. Clearly, there is much work to be done!

Contributor

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